larval phase, rather than in the late larval or the adult stage, as in the gall-forming hymenoptera and the plant-lice. It may be that this new method of vegetative propagation of the larvæ of parasitic insects should receive a distinct name. To call it polyembryony is to mislead all who know what is meant by polyembryony in the original application of this term among plants.

By recognizing the analogy with the previously known instances of alternation of generations among insects the new discoveries become much more interesting than when interpreted as polyembryony. These chains of larvæ correspond to the series of so-called parthenogenetic females of the gall-insects and the plant-lice, which are propagated by vegetative budding. If the fact that all the individuals of the same larva-chain are of the same sex represents a general principle of sex-determination, we must expect to find that this is also true of all the other series produced by vegetative budding. Not only will all the sexual offspring of an individual bud-propagated plant-louse be of one sex, but all the offspring which arise from each original egg of a sexual female. This would mean that what we now call parthenogenetic females are not really females, but represent the two sexes, both temporarily propagated by budding instead of by sexual reproduction. In support of this interpretation we have the fact that even the wingless, larviform, bud-producing plant-lice which are classified as the same species may show two distinct forms, as in the cotton aphis. It now becomes justifiable to suspect that these two forms may represent males and females, and that there may be explained in this way a larval dimorphism which previously appeared altogether mysterious. Adult female plant-lice are wingless, as in all the related families, but the members of these bud-propagating generations are often winged. This has made it necessary to believe that larval females might be winged while the adult females were always wingless, a unique and highly anomalous assumption which the present considerations may enable us to avoid. It is possible that the bud-producing winged insects may prove, after all, to represent the male sex, in spite of their apparent parturition.

One more confusion of terms remains to be noted. The so-called parthenogenesis of these bud-producing plant-lice is entirely distinct from the parthenogenesis of male hymenoptera from unfertilized eggs. If bud-propagation of insects is to be reckoned as parthenogenesis, then the hymenoptera have two forms of parthenogenesis, one a method of sex-determination, the other a method of vegetative multiplication. The hemiptera are now known to have a different method of sex-determination, by means of specialized chromosomes and two or more kinds of spermatozoa, so that the plant-lice should not be expected to agree with the parasitic wasps in sex-determination, even though the methods and results of vegetative propagation should prove to be entirely analogous in the two groups.

Mr. W. F. Wight read the last paper, entitled 'The History of the Cowpea and Its Introduction into America.' This will be published by the U. S. Department of Agriculture.

M. C. Marsh,
Recording Secretary

Discussion and Correspondence

Reversion Induced by Cross Breeding

Dr. Castle[1] in an explanation for reversion, thinks that the wild agouti color has been introduced through the red parent (when crossed with black); and bearing on this is Dr. Davenport's explanation of reversion to Gallus bankiva color, in a cross with white and black poultry, their difference being that Dr. Castle ascribes the reversion of wild color as being added to, and Dr. Davenport as being taken from.

The latter, in his address at the New York meeting, December 28, explains the phenomena of wild color as being due to the absorption of black by the white, leaving the wild color clean. Perhaps both are right in their respective examples.

We wish to tell of some cases, wherein neither of these explanations can be applied as a cause for the reversions.

[1] Science, January 25, p. 151.

Mr. John Chalfant, of Union, Nebr., owns two sows, that from a mating with the same male farrowed seven and eight pigs, all of which bore the seven longitudinal stripes belonging to the young of the wild breeds. The parents—both sire and dams—were three fourths Berkshire and one fourth Poland China, of pure ancestry; and were all black with white points the same as the pure animals of both breeds. When these fifteen striped pigs were ten days old, we predicted to Mr. Chalfant that they would lose their stripes under three months of age, as do our wild pigs (*Scrofa*) at Bear Creek. This proved to be true. Mr. Chalfant was kind enough—at our suggestion—to mate a pair from these reversion litters; and has recently reported the progeny. We quote him verbatim:

> One of the young sows that was striped has farrowed seven pigs, as follows: two black and white spotted, with sandy stripes running lengthwise; one black, with sandy stripes; the other five—nearly black, but with solid, sandy tinge. * * * Surely this must be a reversion of a long way back.

We have here, in this one litter, near and remote reversions; representing three stages, viz., the spotted pigs, representing the Poland China of thirty years ago; the black, with sandy tinge, showing the primitive Berkshire breed of sixty years; and the longitudinal, sandy stripes are a wiping out of all color characters accumulated during domestication.

Agreeing with this is a statement of Mr. John P. Ray, of New York, in conversation with the writer. Mr. Ray originated the Silver Laced Wyandotte twenty-five years ago. Twenty-one years later, he purchased a first-prize cock that had been bred away from his own strain fifteen years. All the progeny from this cock were high-scoring and uniform; but in the product of his daughters, mated with the original strain, were nine pure whites among a hundred chicks.

Another exaggerated reversion occurred at Bear Creek, in pure Short Horn cattle. Two white calves were born, having red ears like the wild cattle of Chillingham. Their sire was a red bull and a first-strain cross between English and Kentucky families. Their dams were both of straight Kentucky strains and both red.

The patience could be overtried with instances of similar phenomena.

In the pig reversions there was a hybridizing not of color, but of chemical. The Wyandottes were of two pure strains in the same breed, and if we may judge from the reputation of their breeders, they were identical in the spots and barrings of every feather.

In the example of the 'wild-color' calves, the parents were identically red in color, were pure registered Short Horns; but three-quarter *strain* hybrids.

We can not believe that this can be accounted for, either by 'adding to' or by 'absorption'; and if your readers will kindly allow, we will propose a solution.

Surmising that heredity in the chromosome is chemic, or enzymic, and in normal line breeding this chemical is stable and repeats itself, but when precipitated by a dynamic reagent the recently acquired less stable hereditary compounds are thrown down, leaving only the old type in many instances. In other cases a new chemical arrangement is produced, as in Burbank's creations and our own swine hybrid monstrosities.

Both Castle's and Davenport's explanations seem logical to their examples, as neither experiment had reached the 'breaking up' stage, wherein we appear to get precipitated reversion.

Castle states that 'ordinary black individuals, while homozygous, are not pure in the sense that they contain no other pigment but black'; and if we understand him correctly, red or brown lies latent. If there were only color reversion, this might apply; but there are also some wonderful morphological reversions to ancestral types, that can only be interpreted as the result of a powerful detergent acting upon the later accumulations of the chromosome.

We are now experimenting with the view of learning rules of hybrid stages of reversion, whereby we can 'break up' or destroy recently acquired characters at will, when these late characters have proven undesirable.

Castle's 'fixation' is good, and the theory

may accomplish much, if he can avoid a precipitate. We hope that when he finds his discoveries are being used, 'the true scientific spirit' will yet allow him to continue his experiments and his philosophy.

Q. I. and J. P. Simpson
Bear Creek Farm

RELATIONS OF SALARY TO TITLE IN AMERICAN UNIVERSITIES

Having just read the interesting discussion in Science of February 15, under the above title, I venture to suggest that there is yet more to be said.

University men possess different kinds of value, *e. g.*: (1) Some are principally of value to the student body in attendance on their classes. (2) Some are valuable more particularly to picked individuals in the university and out of it. Such may be said in a certain sense to have as many students as those whose classes are thronged, but they are in many places. (3) Some will be exceedingly valuable to posterity, but their work is comparatively useless to the present generation, because it has not learned to value or use it, or because it will only reach its greatest significance and utility after it has been carried on for two or three generations.

From the standpoint of the state, all these classes of men are of value and should be supported. If there is any difference in their value, no doubt the pioneers, those of the third class, are the most valuable; but it requires very little reflection to see that these, from a psychological necessity, will be the *least* valued by presidents, trustees and the community at large. To properly estimate the value and importance of an 'infant industry' in the intellectual field requires imagination of such quality that those doing the work do not always possess it, and outsiders almost never.

It is quite possible to argue that the concern of the university is only with the students in attendance, so that all values must be determined by the standard applicable to the first of the above classes. This notion, however, is surely passing away, and with it the possibility of correctly estimating the money value of university men. The larger outlook also serves to convince us that the actual worth of certain professors, having in view their total influence upon contemporaries and posterity, exceeds any sum that can be thought of as payment. On the other hand, the *needs* of the great and the small are not so very diverse.

There is one kind of payment which should no doubt differ greatly according to the character of the man and his work. This is for the support of the work itself. One man may need expensive apparatus, or journeys to distant lands, while others may have no use for these things. This is not necessarily dependent in any way on the eminence of the man himself, but rather on the character of his labors; only, of course, he should be able enough to use well the means provided.

T. D. A. Cockerell
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February 18, 1907

SPECIAL ARTICLES

RIVER CAPTURE IN THE TALLULAH DISTRICT, GEORGIA

The head-waters of the Savannah River have been frequently referred to as an example of drainage transferred from the gulf system to the Atlantic through the process of stream capture. Dr. C. Willard Hayes, in his paper on 'The Southern Appalachians,' published as a National Geographic Monograph, cited this case as an instance of recent capture and ascribed the falls on the Tallulah River (one of the head-waters of the Savannah) to the fact that the newly acquired drainage had not been in possession of the captor sufficiently long for the falls to be worn down to grade. In a paper entitled 'Drainage Modifications' (*Jour. Geol.*, 1896) Mr. M. R. Campbell notes this capture under the heading 'remote changes shown in the streams of the Atlantic slope.' Mr. Chas. T. Simpson (Science, 1900), in discussing 'The Evidence of the Unionidae regarding the Former Courses of the Tennessee and other Southern Rivers,' reports the finding of mollusks similar to the Tennessee and Coosa River forms in the